THE HARVEIAN ORATION
1873

GEORGE ROLLESTON

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THE HARVEIAN ORATION

1873.

BY

GEORGE ROLLESTON, M.D., F.R.S.,

LINACRE PROFESSOR OF ANATOMY AND PHYSIOLOGY,

AND

FELLOW OF MERTON COLLEGE, IN THE UNIVERSITY OF OXFORD.

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Τί τόδ' ἄχνυμαι

φθόνον ἀμειβόμενον τὰ καλὰ ἔργα;

φαντί γε μὰν οὕτω κεν ἀνδρὶ παρμονίμαν

θάλλοισαν εὐδαιμονίαν

τὰ καὶ τὰ φέρεσθαι.

PINDAR, *Pyth.* vii. 19.

TO

GEORGE BURROWS, M.D., F.R.S., D.C.L.,

PRESIDENT OF THE

ROYAL COLLEGE OF PHYSICIANS OF ENGLAND,

WHOSE OWN ATTAINMENTS,

WHOSE SYMPATHY WITH THE INTELLECTUAL ACTIVITY

AND PROGRESS OF OTHERS,

WHOSE UNSWERVING DISCHARGE OF GREAT, AND WHOSE

CONSCIENTIOUS FULFILMENT OF DETAIL DUTIES,

FURNISHED FOR MANY YEARS A VALUABLE EXAMPLE

TO THE

STUDENTS OF ST. BARTHOLOMEW'S HOSPITAL,

THIS HARVEIAN ORATION

IS GRATEFULLY DEDICATED

BY ONE OF HIS FORMER PUPILS.

OXFORD, 1873.

THE HARVEIAN ORATION

1873.

Mr. President and Fellows of the College of Physicians:—A man whose lot it is to live away from London may well feel some diffidence in accepting an invitation to lecture before a metropolitan audience; and, Sir, when you honoured me by requesting me to deliver this year's Harveian Oration, I felt and expressed this natural hesitation. I wish to record that you pointed out to me that my function in Oxford was to pursue and lecture publicly upon the very subjects with which Harvey occupied himself; and I suggested to myself that what could with any propriety form the substance of a course of lectures in the one place, could, *mutatis mutandis*, furnish materials for an address in the other. I felt besides, that, as the President of the College of Physicians is by virtue of his office one of the five electors

to the Linacre Professorship, the Linacre Professor might seem scarcely justified in declining an invitation to appear before the learned body to which in part he owed his position; and, though I mention it last, I felt first of all that a wish expressed to me, not so much by the official whom I am now addressing, as by the individual who now more than twenty years ago introduced me to Harvey's hospital, and has persistently befriended me ever since, was a wish which I ought not lightly to disregard. If now, Sir, I follow an example which you have often set me, and, without needless preface or further personal allusions, address myself at once to the business before me, I shall thereby pay you the best of all compliments, by showing you that your teaching has not been wholly thrown away upon your former pupil.

The time allotted to me I propose to occupy, firstly, in expounding with all possible brevity certain advances recently made in our knowledge of the anatomy and physiology of the circulatory organs; and, secondly, in giving the as yet unrecorded history of one

of the many attempts to rob Harvey of his rightful rank in the noble army of discoverers, which were made in the latter half of the seventeenth century.

Some of the last, if not the very last, of the many fruitful experiments which Harvey performed in the way of interrogating Nature as to the circulation, were experiments in the way of injection. If the writer of a work which appeared but some forty-three years ago, On the Diseases and Injuries of Arteries[a], had taken the pains to repeat those experiments which Harvey performed more than two hundred and twenty years ago, and when in his seventy-fourth year, we should not have had the following statement at page 9 of his book: 'I have conceived that the arteries contain air in an uncombined state, which may assist in keeping them distended, and in facilitating the circulation; but I have not

[a] On the Diseases and Injuries of Arteries, with the Operations required for their Cure; being the substance of the Lectures delivered in the Theatre of the Royal College of Surgeons in the spring of MDCCCXXIX. By G. J. Guthrie, F.R.S. London, 1830.

been able to prove it.' The fact that Harvey performed experiments in the way of injection may be unknown to many persons who are too well informed to conceive that the arteries may or can, compatibly with the carrying on of any circulation, contain air in an uncombined state; for these experiments are not to be found recorded either in the treatise De Motu Cordis or in either of the two letters to Riolanus; which two compositions were, in the older editions of Harvey's works, printed as three parts of a single treatise, under the names of 'Exercitatio Anatomica i. De Motu Cordis, etc.,' 'Exercitatio Anatomica ii. De Circulatione Sanguinis,' and 'Exercitatio Anatomica iii. De Circulatione Sanguinis'; and were, till the appearance of the College of Physicians' edition in 1766, the only published[b], as they

[b] The statement made (by Dr. Akenside; see Pettigrew, 'Medical Portrait Gallery,' Preface, p. 7, citing Dr. F. Hawkins) in the Praefatio to the College of Physicians' edition of Harvey's works to the effect that only two of Harvey's Letters had been published prior to the year 1766, is not correct. Horstius, as Harvey's words in the Epistola Sexta, p. 631 (Harveii Opera, ed. 1766) show, when read in connexion with the Epistola immediately

are still the best-known, records of Harvey's work and labour upon the circulation of the

preceding it, received three letters from Harvey. By consulting Horstius' work referred to by Dr. Akenside, *l. c.*, I found at pp. 61–65 the letter, which appears in our edition as 'Epistola Tertia responsoria Morisono,' published by Horstius in 1656 with the omission of the first six and a half, and also of the last three and a half lines. These lines Harvey had doubtless ordered his amanuensis —a functionary of great importance to one who wrote so bad a hand (see p. 165, ed. 1766, or Harvey's own autograph MS. No. 486, Sloane Coll. British Museum) —to omit when he bade him copy and send to Horstius, 'eadem quae antea medico cuidam Parisiensi (sc. Morisono responderat.' Horstius does not publish Harvey's letter (the 'Epistola Quinta' of our edition) of date Feb. 1, 1654–5, but appends the last letter of the three (the 'Epistola Sexta' of our edition) to his own answer to Harvey's earlier communication. I shall henceforward refer to the College of Physicians' edition of Harvey's works as 'ed. 1766,' and to Dr. Willis' most valuable translation of them, published by the Sydenham Society in 1847, as 'ed. Willis.' I throw out as a topic for future discussion the question whether Dr. Willis is right in following the editions of Harvey's writings of an earlier date than 1766, in retaining the negative in the sentence (at p. 131 in both his edition and in that of 1766) in the second epistle to Riolanus which refers to the Critias of Plato. I think Dr. Willis is right, and that Dr. Lawrence was wrong; but to do this it is necessary to sacrifice Harvey's credit for knowledge of Plato whilst vindicating the consecutiveness of his reasoning. Harvey himself

blood. The experiments to which I refer are put upon record in a letter of Harvey's to P. M. Slegel, of date 1651 (see Harveii Opera, ed. 1766, p. 613; ed. Willis, p. 597). They were undertaken with the object of giving a final and happy despatch to all the quibbling objections of Riolanus, 'omnes Riolani circa hanc rem altercationes jugulare;' and they consisted, firstly, in forcing water from the cava into the right ventricle whilst the pulmonary artery, the 'vena arteriosa' of those days was ligatured—whereby Riolanus' suggestion as to the permeability or porosity of the interventricular septum was shown to be untenable; and, secondly, in forcing water from the pulmonary artery round into the opened left ventricle, whereby the lesser circulation was demonstrated, to use Harvey's own favourite word, *αὐτοψία*; or, to use the very words employed by him upon this very oc-

would probably have accepted this alternative. It is right to add, however, that so far as my reading of the edition of 1766 has carried me, I have come upon no other case where I have been forced to think that Dr. Lawrence may have blundered.

casion, by an 'experimentum ἄφυκτον a me' (in his seventy-fourth year) 'nuper et collegis aliquot praesentibus exploratum.' Simple as this experiment may seem to us now, I do not think that any apology is required for the drawing of attention to it; for it is only twenty-eight years ago (see Edinburgh Medical and Surgical Journal, vol. lxiii. p. 20), that Dr. Sharpey, to whom our Baly Medal has been so recently and so fitly assigned, had to perform the very closely similar experiment of injecting defibrinated blood into the thoracic aorta, with the very closely similar object of showing that the force of the heart was sufficient to account for the passage of blood through the intestinal and hepatic vascular systems—nay, to perform an all but identical experiment, adding on to it but the means for estimating and reproducing the force put out by the ventricle concerned. If such experiments as these were necessary in 1845, how much more necessary must have been the still simpler experiments of Harvey in 1651! At that time, the prestige of Riolanus the younger 'pressed heavily upon mankind.'

Harvey himself had called that individual 'anatomicorum coryphaeum' in 1649; and, in the very year and letter we are dealing with, he calls him 'celebrem anatomicum.' And Pecquet, the discoverer of the thoracic duct, in his work, also of this selfsame year 1651, the Experimenta Nova Anatomica, a work spoken of by Haller (Bibliotheca Anatomica, i. p. 443) as 'nobile opus et inter praecipua saeculi decora,' has the following remarkable passage: 'Ita sentiunt non vulgaris peritiae medici Harveius, Veslingius, Conringius, Bartholinus, aliique complures; nec melior ipse Joannes Riolanus (quod mirari subit pro eximiâ viri, quâ in rebus anatomicis caeteros anteivit sagacitate). Audi hanc in rem illius sententiam.' p. 4, *l. c.* This, I think, I will spare you; but I will remark that, after this singular—or perhaps, alas! not singular—instance of the blundering judgments which contemporary writers may pass upon each other, no young man, nor indeed any old one—for Harvey was in his seventy-fifth year when he first read Pecquet's work (see Ep. Tert. p. 620, ed. 1766; p. 604, ed. Willis)—should overmuch

fret if his own age, in his own estimation, do him scanty justice. Posterity ordinarily—I do not say always — rectifies these false judgments; it has done so, at all events, in the cases of the men so grotesquely grouped together by Pecquet[c]. Haller, for example, writing in 1774 (Bibliotheca Anatomica, i. p. 301), speaks of Riolanus as 'vir asper et in nuperos suosque coaevos immitis ac nemini parcens, nimis avidus suarum laudum praeco, et se ipso fatente anatomicorum princeps.' The duty of attacking and abolishing such a man may, or indeed must have been, a disagreeable one to his contemporaries. They appear to have shirked it: it was their duty to have faced it, notwithstanding it might have been disagreeable.

Harvey used for these experiments a somewhat rough injecting apparatus, 'quemadmodum in clysteribus injiciendis fieri

[c] See also, I would add, Gregorius Horst, the father of Harvey's correspondent of the same name, in his Opera Medica, i. p. 83 (1661), where Riolanus is spoken of as 'anatomicorum hujus saeculi fere primum;' and consult Bartholinus himself, who, in his work De Lacteis Dubia (1654), refers to 'multis Riolani observationibus quibus rem anatomicam immortali nominis celebritate auxit.'

solet' (p. 614, ed. 1766; p. 597, ed. Willis). The modern experiment which I wish first to introduce to your attention, rests for its accomplishment upon the employment of the delicate injection-syringe (for *Einstichung*) of Ludwig, and of the fine soluble Berlin blue for the substance to be injected. Here, as in many other instances, our superiority to our forefathers rests mainly or wholly upon our possession of more delicate, or upon our command of more powerful agents; and the delicate syringe and the penetrating soluble injection-mass help us to discoveries and demonstrations impossible in default of such means; just as the superior lenses of Malpighi and Leeuwenhoeck helped them to the discovery and demonstration of the capillary circulation, unknown to the discoverer of the makroscopic circulation. The experiment to which I refer has its results fairly represented in the accompanying drawing Fig. 1) of a specimen prepared by myself at a class-demonstration. It gives a figure of the lacteals injected, by the means just specified, as they exist upon the terminal segment, here widely globular, of the ileum,

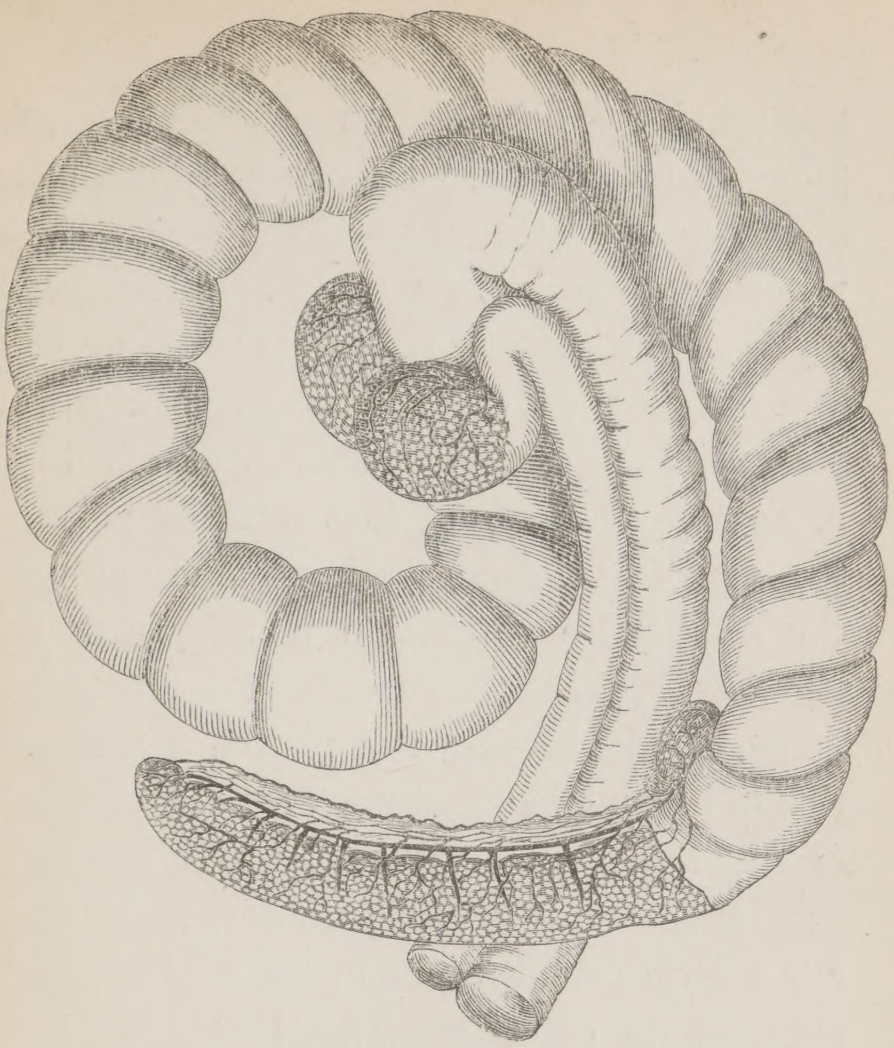

FIG. 1.—Vermiform Appendix, Caecum, and parts of Small Intestine and Colon of Rabbit (*Lepus cuniculus*), with the Peyerian and also some of the mesenteric glands in connexion with them injected. The ileum and colon lie side by side, the former describing a siphon-shaped curve before ending in a dilatation, known as the '*sacculus rotundus*,' and homologous with the aggregation of Peyerian follicles situated in man just anteriorly to the ileo-caecal valve. The *sacculus rotundus* occupies the centre of the figure; above and a little to the left of it another somewhat similar aggregation of Peyerian follicles is seen just beyond the ileocolic valve. The colon curves backwards externally to and concentrically with the ileum. The caecum consists of two portions: one larger in calibre, thinner in walls, and sacculated spirally; the other, the homologue of the human 'appendix vermiformis,' smaller in calibre, but with much thicker walls. It has been injected as described in the text, and the injection is seen to have passed into some mesenteric glands situated close to the junction of the two segments of the caecum, and between it and the colon.

upon a single disciform patch upon the commencing colon, and, finally and chiefly, upon and all around the walls of the colossal vermiform appendix of the rabbit. In this latter place it is but what the Germans call, and have called (Frey, Das Mikroskop und die microskopische Technik, 4th ed. 1871, p. 255), a *Kinderspiel*, to insert the point of the fine *Einstichung* syringe charged with the soluble blue injection just beneath the peritoneal coat at the caecal end or elsewhere, when, upon pressing the piston, a reticulation of blue will spread itself over the surface of the tube, enclosing as islands the solid substance of the Peyerian follicles. It needs but a little perseverance in the way of gentle pressure to cause superficial tubular lymphatics to arise into view, and to declare their true character by their contrast with and distinctness from the blood-vessels, as well as by their moniliform character speaking of their richness in internally placed valves. Passing over the convex walls of the appendix, they join larger trunks which run along its mesenteric border ; these larger trunks in their turn enter the mesenteric

glands, and form in their substance reticulations strikingly like those formed previously in the walls of the intestine around the solid substance of the Peyerian follicle—suggesting thus to the naked eye the similarity, and by consequence the homology, which a microscopic examination enables us to prove to exist between the lymph-sinuses and the solid masses they surround in the Peyerian follicles and in the mesenteric glands respectively [d].

It is the demonstration of the relation of the lymphatic or lacteal vessels, or sinuses, as the case may be, in different animals, to the solid ampulla-like masses in the Peyerian

[d] I take this opportunity of expressing my surprise that Henle has not seen his way towards accepting this view of the real nature or *Bedeutung* of the Peyerian follicles. In his 'Gefässlehre' of 1868 (p. 404) he refers us back to his 'Eingeweidelehre,' p. 57, of 1862, where, as also in the second edition, 1873, p. 62, the absorbent character of these structures is denied, just as it was by Hyrtl in his 'Handbuch der Topographischen Anatomie,' 1860, p. 646, and by Teichman, 'Das Saugadersystem,' 1861, pp. 86–91. The view which I have adopted was accepted by a distinguished Fellow of this College, Dr. Burdon-Sanderson, in the Eleventh Report of the Medical Officer of the Privy Council for 1868, p. 96.

follicles, which the modern method of puncture can claim as being eminently its own attainment; for many years ago—in 1784, in fact—and three years before the appearance of Mascagni's splendid work, with similar figures and histories of similar experiments (Vasorum Lymphaticorum Historia et Iconographia, 1787), the continuity of the lacteal radicles upon the walls of the intestine with the 'lymph-paths'—to borrow a word of later coinage—in the mesenteric glands, and finally, after passing through successive lines of these apparently solid structures, with the thoracic duct itself, had been demonstrated by Sheldon, then Professor of Anatomy in our Royal Academy of Arts. These are his words (from p. 49 of his work, Of the Absorbent System, 1784), describing his plate No. 5, a copy of which I have had made and suspended here: 'In the fifth plate of this work, upon a portion of human jejunum from an adult female subject, seventeen lacteal vessels are injected with quicksilver, by inserting pipes into them upon the intestine. They were remarkably large and varicose in this subject,

and as the quicksilver was poured into the lymphatic injecting-tube to fill these vessels, it frequently ran out in a full stream by the jugular vein, which was opened. This circumstance rendered it evident that the mercury had passed through the whole course of the lacteals and thoracic duct, and had penetrated even into the venous system. It is, I believe, the only instance in which the thoracic duct has been injected from the lacteals on the intestines[e].

[e] I have some pleasure in pointing out that by making a reference to the plates of the venerable Professor Arnold, Fasc. i. Tab. i. fig. 2, 1838, it may be seen that the quicksilver injection could sometimes give as correct results as the 'silver method' of modern microscopy for the detection of lymphatics by their epithelium. The figure I refer to shows the fourth ventricle plexus without, the velum interpositum, on the contrary, with, lymphatics injected with quicksilver. The use of the silver method has enabled me to prove that this representation is correct: abundance of choroidal villi can be procured, and very beautiful objects they are when treated with 0.25 per cent. solution of nitrate of silver, from the plexus in the fourth ventricle, but no lymphatic vessels. These can be shown from the velum interpositum by the use of the same reagent. The use of quicksilver as an injection-substance has not always led to as happy results as in the instance just given. Not to specify other cases, it is curious to note that the penetra-

Sheldon's first plate, I may add, when compared with his letter-press on p. 37, appears to show that what he calls 'ampullulae' were really Peyerian glands, and that he had repeatedly seen these glands distended in the way of natural injection with chyle, as it is easy enough to see them distended in an animal, such as a rat, which can be got to feed on fatty food, and can be killed at a proper interval of time afterwards. He appears to have had very serious as well as reasonable doubts as to the existence of any foramen in the apices of these 'ampullulae'; but the authority of Lieberkühn, whose Dissertatio Anatomica (p. 18) he had himself edited, appears to have weighed with him more than his *αὐτοψία*. Near, therefore, as Sheldon came to seeing the whole truth, he just failed of

tion of this material into the parieto-splanchnic ganglion of the Lamellibranchiata when thus employed by the skilful Italian anatomist, Poli, and its distributing itself thence into the nerves given off from it by displacement of their granular neurine, seduced him into supposing these structures to constitute a lymphatic system, 'cisternam lacteam et vasorum lactiferorum surculos.' See Testacea Utriusque Siciliae, tom. i. p. 39, 1791.

doing so entirely and completely; and the views which Lieberkühn had put forward (p. 10, *loc. cit.*) as to the great number of the Peyerian glands in the lower segment of the small intestines, being a proof that they held relation to secretion or excretion rather than to absorption, prevailed and have prevailed, even into our own day. These are Lieberkühn's words: 'Quare ad finem ilei plures quam in integro intestino positi erunt? Nonne propter faeces jamjudum exsuccas et indurescentes ut lubricatae valvulam facile transeant nec laedant?' In Henle's ordinarily and marvellously excellent Generelle Anatomie, of date 1841, I find (p. 895) the excretory character of the Peyerian follicles taken as something certain; the only thing left uncertain being the question as to whether their contents found their way into the cavity of the intestine by a constantly patent, however small, duct, or by dehiscence, as ova from an ovary. In 1850 the real meaning, the true physiological import, of these glands was proved by Brücke. The method of injection, of which I have spoken, enables us

to demonstrate or exhibit what was then proved, and that with the greatest ease. It is difficult to understand how any one can now doubt that the Peyerian glands are really but the *pileorrhizae* of the roots, the glands the *tubera* and the thoracic duct the trunk or stem of the absorbent tree.

If any apology be needed for my dwelling so long upon a point of anatomy which has not merely much historical, but also much practical, interest—the Peyerian glands being the part of the organism especially affected by the poison of typhoid fever, which I see has, amongst other *aliases*, that of 'Peyerian fever' (Walshe, On Diseases of the Heart, 3rd ed. p. 208)—I would add that I was till recently under the impression that the actual demonstration, the doing, of that which my Fig. 1, p. 17, represents as done, might have been a fitting exhibition for me to go through upon the present occasion, following herein the example of Harvey, 'viliora animalia in scenam adducentis.' I have, however, learned that this very demonstration on the appendix vermiformis of the rabbit has been often performed in Germany, and,

indeed, also in England; and I judged, consequently, that it might be superfluous, as it would not be novel, to exhibit it here and now.

Having been thus disappointed in my intention of demonstrating something new in this direction, I cast about in another for something of the same character. And in the heart of a bird, the Australian Cassowary (*Casuarius australis*), killed at Rockingham Bay, lat. 18 deg., on the east coast of that continent, and sent me by my former pupil, J. E. Davidson, Esq., I came upon a structure which I am well assured has never been either described or figured before. It possesses upon this ground some claim upon our attention; but it possesses stronger claims than any which mere rarity could give it, being a structure which, though it has never been seen in any other member of the class Aves, is largely developed, and, indeed, exactly reproduced in the hearts of certain mammals, and does not fail to be represented, at least rudimentarily, in our own. The structure in question is a 'moderator' band, holding precisely the same

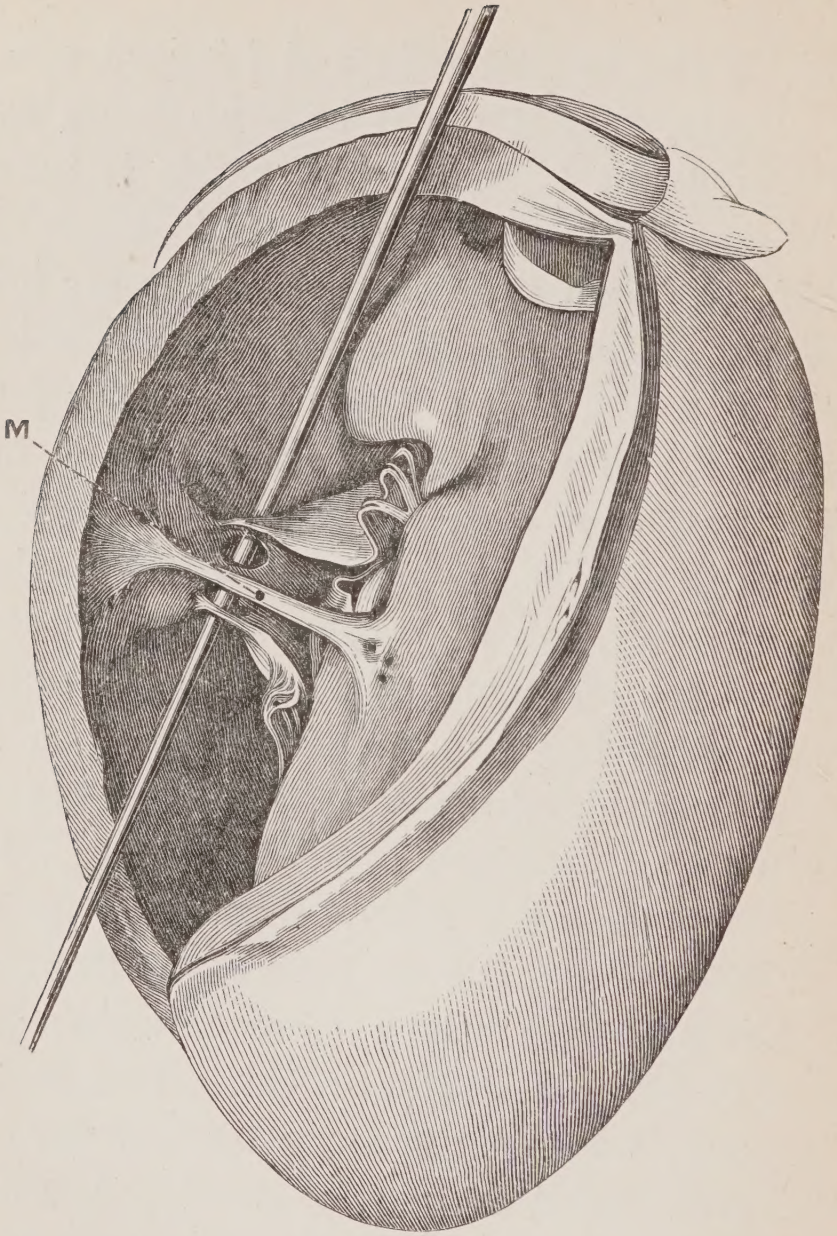

Fig. 2.—Heart of Sheep. The right ventricle is laid open. The letter **M** indicates the moderator band. A probe has been passed between the chordae tendineae, passing from a musculus papillaris arising from the movable wall of the ventricle to the two main segments of the tricuspid valves and the outer wall of the ventricle.

relations to the other parts of the right ventricle in this bird which the band so named by Mr. T. W. King in the Guy's Hospital Reports, vol. ii. p. 122, 1837, holds in many, if not in all, Ungulate mammals. This, I presume, is made plain by a comparison of the two diagrams (Figures 2 and 3) showing, one of them the heart of this bird, the other the heart of a sheep, with the right ventricle similarly laid open in each case. The advantage, which in the struggle for existence, and specially in that very common phase of it which takes the form of a race for food or from an eater, which an animal with such a muscular band passing directly across the cavity of its right ventricle from its fixed to its movable wall must possess, is not a difficult thing for any man to understand who has ever either watched in another or experienced in himself the distress caused by the over-distension of any muscular sac[f]. A band of similar function

[f] Since writing as above I have been reminded of what I ought not to have forgotten, viz. that my friend Dr. Milner Fothergill has discussed this very subject in his work, The Heart and its Diseases with their Treatment, London. 1872, p. 6.

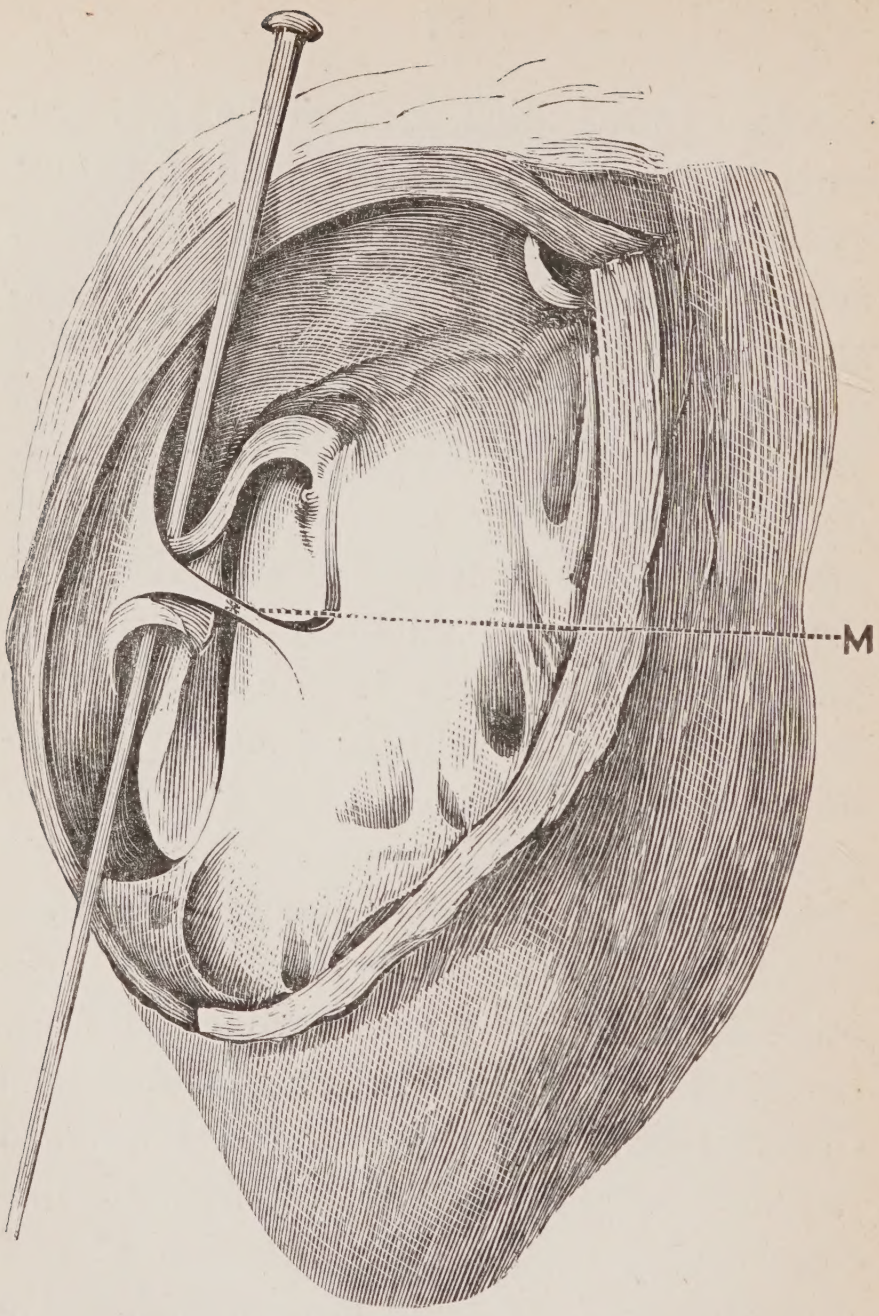

FIG. 3.—Heart of Australian Cassowary (*Casuarius australis*), prepared similarly to the heart of sheep, figured in Figure 2, and showing at M the moderator band. A line of papillary upgrowths are indicated at the upper end of the upper segment of the muscular auriculo-ventricular valve, passing downwards towards the point of origin of the moderator band from the septum. They extend in the specimen over a space of about three-eighths of an inch, and are indicated in the woodcut by dark transverse lines.

—I do not say definitely of precisely the same morphological importance—has often been figured as existing in the hearts of most or all Reptilia below Crocodilina; and it serves in them to close up and expel the blood from the pulmonary compartment of their imperfectly divided ventricle.

Such being the function of this moderator band, what is its morphological bearing, and what traces can we find of it in ourselves, tempting us to speculate as to the nature of the secret bond which brings us into relations of affinity not only to the mammalian class, but with an older stock, the many-sided potentialities of which embraced not only mammals, but all warm-blooded animals, and not only all warm-blooded creatures, but warm-blooded animals and reptiles also? The valves of the heart in the higher vertebrata, when regarded from this point of view of development—the safest if not the sole criterion of homology—may be spoken of as being but trabeculae flaked off from the inner surface of the wall of a muscular sac, and subsequently made more or less membranous in the way of specialisation and its

correlative economy. Thus, as Gegenbaur (Vergleichende Anatomie, 2nd edition, p. 836) has remarked, the intervalvular space in these animals corresponds to the entire cavity of the spongy walled heart of fishes and amphibia; and the sinuous intertrabecular cavities in the spongy walls of these latter animals correspond with the chief part—viz. the extravalvular part of the ventricular space—in mammals, birds, and Crocodilina. Now, the musculi papillares represent the disposal or destination of the innermost layer of the right ventricle, according to Dr. Pettigrew (see his paper, Phil. Trans. 1864, p. 479); and I would submit that the moderator band is but a specialisation of the next layer in order from within outwards — to wit, Dr. Pettigrew's sixth layer, which he has figured (Plate XIV. fig. 33) as proceeding in a spiral direction from right to left, much as the fibres of the moderator bands I have figured do. A study of the heart of the rabbit will put this matter in a very clear light, and further open our eyes to see and recognise the rudimentary representation of this

moderator band in our own hearts. If we look at the outer aspect of that very constant musculus papillaris, which passes in man from the outer and movable wall of the right ventricle to distribute its chordae tendineae to the two more anteriorly placed of the three segments of its auricular valve, we shall frequently see that its longitudinal fibres are crossed nearly or quite at right angles by a slender fibrous band, so that we have before us an appearance not wholly nor essentially unlike that presented by the striae longitudinales of Lancisi and the fibres of the corpus callosum when viewed in their mutual connexion. This band of fibres can sometimes be traced up towards the conus arteriosus, and be seen not to die away until close upon the point of origin of the most anteriorly or upwardly placed of the chordae tendineae which arise from the septum to pass to the hindermost of the three segments of the tricuspid. The points between which this line of fibres lies may be observed to be the very same as those between which the moderator bands in the Cassowary and the Sheep stretch as free columns in the

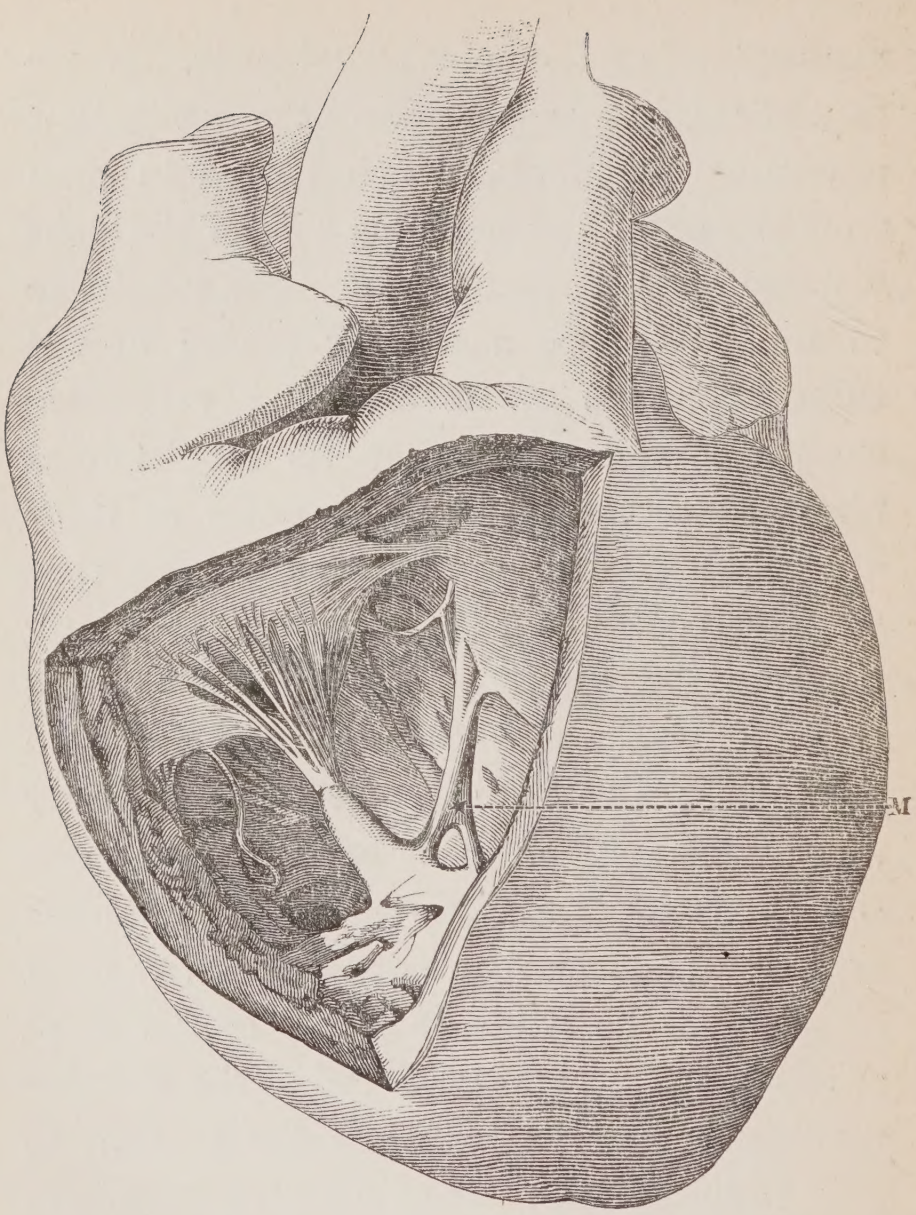

Fig. 4.—Human Heart. The right ventricle has its cavity exposed. The moderator band M is seen to pass up from the base of a musculus papillaris arising from the outer or movable wall of the ventricle into the neighbourhood of the conus arteriosus. Close to this, its upper end, a chorda tendinea arises by two roots, and passes up to the most posteriorly placed of the three cusps of the tricuspid valve.

diagrams before you. It is not altogether rare to see this band raise itself from the position of fusion, like the ventricular wall, and assume the character of a cylindrical band for a lesser distance, but with no less distinctness as a column, than in the Ungulata. Such a case I had actually before me whilst writing this, and you have it now figured before you (Fig. 4) g.

Every gradation, in fact, exists between the entire obsolescence of the moderator

g Since this oration was delivered I have received two communications relating to the presence of a moderator band in the human heart. One of these, from my former pupil, J. C. Galton, Esq. F.L.S., was accompanied by a sketch in which a moderator band was drawn as passing in a human heart from the insertion into the movable wall of the ventricle and the very constant musculus papillaris supplying what I would call the '*conad*' and '*dextrad*' cusps of the tricuspid, to an origin on the interventricular septum, sending a root up to the point of origin of one of the chordae tendineae of the third cusp, called '*septal*' by Mr. Galton. See also Mr. Galton's Letter to British Medical Journal, July 26, 1873, p. 83. I have to thank Dr. Headlam Greenhow for a reference to another notice of the presence of a moderator band in a human heart. It will be found in an interesting paper of his in the Transactions of the Pathological Society, vol. xxi. 1870, p. 88.

band, which we sometimes see in the human heart, through the typical, and I should anticipate, constant, but not functionally important, representation of it in the rabbit, up to the important and structurally prominent development attained to by it in the Ungulate mammal, and this solitary instance for the class of Birds, and the sub-class with such generalised affinities, of Struthiones.

And, speaking of the method of gradations, I take this opportunity of saying that its application in the case of the muscular right auriculo-ventricular valve of birds will, in my judgment, put an end to the disputes which have taken place as to its homology with one or other of the two valves in the crocodiles. The two portions of the valve in the *Casuarius australis* are so nearly equal—the larger being 1·7 inch, as against 1·4 of the smaller—as to do away with the difficulty which might be felt in holding that both Crocodilian valves are represented here. There are other reasons for this view, which I reserve for another occasion. But whilst speaking of the heart of the Bird, I cannot forbear pointing out how the structural ar-

rangements of its auricle, differing as they do strikingly from those of the same compartment in the mammalian heart, help us by that contrast to get a true idea of the working of this latter. Firstly, the walls of the Bird's right auricle are relatively thicker, not only as compared with the walls of its own ventricle, but also as compared with the walls of the corresponding auricle in the Mammal, the musculi pectinati standing out in as sharp relief as the similarly working muscular ridges in a hypertrophied bladder, and inclosing anfractuosites and recesses almost as deep. But, secondly, and what is of more importance, the Bird's auricle is furnished with a large and functionally active valve, protecting the entrance of the great veins, and preventing regurgitation into those vessels just as the auriculo-ventricular valves prevent regurgitation from the ventricles. It is fair to argue *à priori* that if the Mammalian auricle had counted for as much in the action of the heart as the Bird's, its force would have been economised by the placing of a large and functionally useful valve in the site of the rudimentary

Eustachian—a structure altogether absent in many mammals, and variable, as rudimentary structures very often are, in ourselves. The *à priori* argument of Comparative Anatomy is abundantly borne out by the appeal to experiment. Marey, in his 'Physiologie Médicale de la Circulation du Sang,' 1863, whilst referring (p. 36) to other evidence from Comparative Anatomy than that which I have adduced, cites, in support of the view that the auricle has but an accessory and subordinate *rôle* in the functions of the heart, an experiment of Chauveau's, in which the auricle of a horse, being exposed and irritated, lost its contractile power for a time, during which, nevertheless, the ventricles continued to contract and the circulation to be maintained. Colin, again (Traité de la Physiologie Comparée, vol. ii. p. 257, 1856), found that the left ventricle continued to be filled with blood even when the corresponding auricle was prevented from contracting by the insertion into it of a finger. And further, Magendie had long ago noted, in experimentation, what many here present may have noted

in pathological or clinical observation—viz. that the auricles may remain extremely distended for hours, and, like other muscular sacs similarly conditioned, unable to contract and empty themselves, without the circulation for all that being brought to a standstill. It was Dr. Pavy's paper, treating (in the Medical Times and Gazette of November 21, 1857) of the case of a man (E. Groux) with a congenital fissure of the sternum, which first drew my attention to these points; and his summary of what takes place in the dog is so clear that I herewith reproduce it.

'In the dog, the contraction of the ventricles is sharp and rapid, instead of prolonged, as in the reptile, and does not appear to occupy nearly so much time as half the period of the heart's action. The ventricular contraction communicates a sudden impulse to the auricles, occasioning in them a distinct pulsation, which is instantly followed by a peculiar thrill, wave, or vermicular movement, running through the auricular parietes down towards the ventricle. This thrill or wave is coincident with the passage

of the blood from the auricle into the ventricle, and takes place so instantaneously after the ventricular contraction, that the one movement appears to run on to continue itself into the other. There is then a pause, which seems comparatively of considerable duration, and which is succeeded by a recommencement of the heart's action, beginning with the ventricular contraction.'

Dr. Pavy has very kindly gone to the trouble of repeating the experiment upon which these statements are based; and from a letter with which he has favoured me, I gather that the auricular contraction detectable by the cardiographic tracing, as immediately preceding the ventricular contraction, is also detectable, of course during the pause just mentioned, by the eye, unassisted by the cardiograph, and turned simply upon the exposed heart, in which the auricular appendix is seen to become redder or more flesh-coloured at the moment in question. And he further remarks that this auricular contraction, difficult[h] though

[h] I apprehend that Dr. Walshe's account of the auscultatory phenomena as occurring under normal conditions

it be to be observed under physiological conditions, may be exaggerated into considerable prominence in disease entailing contraction of the auriculo-ventricular orifices, and may then make itself known by a presystolic murmur.

I should now be glad to draw attention shortly to a few memoirs which have appeared comparatively recently, and which treat of matters of considerable interest, not merely as scientific problems, but also as practical questions. First among these I would name the paper which appears in the third volume of Professor Ludwig's 'Arbeiten,' 1868 (having previously appeared in vol. xx. of Bericht Math.-Phys.-Klass. K. S. Gesellsch. Wissensch., Leipzig), by Professor Ludwig himself and Dr. Dogiel. In this paper we have a number of experiments recorded as performed with the hearts of dogs removed from the body, and as nearly

will be accepted as correct. It runs thus (Diseases of the Heart and Great Vessels, 3rd ed. 1862, p. 65): 'In the normal state the blood enters the ventricles from the auricles with a current so calm as to prevent audible sound from being thereby produced in the former cavities.'

as possible emptied of blood; and the conclusion which the authors come to is that the heart of the dog, when removed from the body and emptied of blood, still produces a sound during the systole of the ventricles which is not essentially different from that which is recognised as the normal first sound of the heart. The authors add, however (p. 85), that they do not think these experiments entirely exclude the possibility of the tension of the auriculo-ventricular valves entering as a factor into the production of the first sound; and hereby they would be guarded from coming into contradiction with most English authorities—as for example, Dr. Walshe (Diseases of the Heart, 3rd ed. 1862, p. 62). Dr. Guttmann, however, in a paper of no great length, but of considerable merit, published subsequently to the one just mentioned, and in Virchow's Archiv for 1869, points out with much acuteness what, when once pointed out, is ever thereafter obvious—viz. that it is, in the nature of things, impossible, with all possible precautions in the way of emptying the heart of blood, to empty the complex

phenomenon made up by a systole of the heart of the condition of tension of the auriculo-ventricular valves. Surely the musculi papillares will contract with the rest of the ventricular walls, and, contracting, will they not stretch the chordae tendineae and the valves? For myself, I would say that we are more likely to overrate the share taken by the valves than to underrate that taken by the muscular walls. I need not say to this audience that the fact with which we are all familiar, of the alteration in the first sound produced by disease of the auriculo-ventricular valves, does not absolutely prove that they produce any part of it during health; and, finally, to my own ear at least, a modification of Wollaston's experiments, which anybody can try for himself by making his temporal and masseter muscles contract at any time of perfect stillness, appears to produce a sound which is scarcely, if at all, different in quality from the first sound of the heart. A judgment, however, upon the nature of a sound, or, indeed, an aggregation of sounds, as in music, is one upon which two observers may

very well differ, as neither of them can lay his proof of supposed identity or difference alongside of that which the other may possess, or may suppose he does.

It is with much pleasure that I refer to Dr. Rutherford's paper on the Influence of the Vagus on the Vascular System, which appears in the Edinburgh Royal Society Transactions for 1870, vol. xxvi. In that year, having to deliver an address to the Biological Section of the British Association at Liverpool, I made bold to say that the results to which Dr. Rutherford had come, and which were then only known to me in an abstract in the Cambridge and Edinburgh Journal of Anatomy and Physiology (May 1869, p. 402), would prove to be of the highest value and importance. His memoir now published *in extenso*, and extending over forty-two pages, as fully justifies my prediction as it will fully repay any one who will take the pleasant trouble of reading it. The most important result in a practical point of view is the demonstration which Dr. Rutherford has given of the nerve-circle, whereby, in the way of reflex action, the all-important

secretion of gastric juice is called forth. The sensory impulse caused by the ingestion of food into the stomach, is propagated upwards by the vagi to the medulla oblongata, where it throws into abeyance the vaso-motor nerve-cells, which, whilst the stomach is empty, keep the blood-vessels of the gastric mucous membrane constricted, but which, when their activity is inhibited, allow the zonular fibre-cells of these blood-vessels to dilate, and allow the increased afflux of blood thus called for. That relief will result to some of the countless martyrs to dyspepsia out of the demonstration of this physiological relation of vagus, sympathetic, and peptic glands, I do not doubt, Possibly, I would add, Owsjannikow's observations as to the working of hydrate of chloral as a depressor of arterial tension (Ludwig's Arbeiten, 1872, p. 32) may prove valuable to persons engaged in practice, by pointing out, in however shadowy a fashion, the road to a more rational and systematised, even if less general use of this drug than that which I am told is now made of it. It may seem a paradox, but it is none the less true for all that, to say that,

for the activity of many organs, a paralysing and inactivity of certain nerve-centres in connection with them is a prerequisite. The activity of such, indeed of most, organs is but intermittent and occasional, being but intermittently and occasionally called for, whilst the constringing activity of the sympathetic has to be constantly at work to prevent waste of force [i].

Owsjannikow's paper (also to be found in Ludwig's Arbeiten, 6th year, 1871, and in the Bericht Math.-Phys.-Klass. K. S. Gesellsch. Wissench., Leipzig) just referred to, and published two years subsequently to Dr. Rutherford's, gives, as the result of a number of experiments performed in Professor Ludwig's laboratory at Leipzig on rabbits, and independently at St. Petersburgh on cats, the conclusion that the ganglionic centres of inner-

[i] The phenomenon of the distension of the corpora cavernosa, a phenomenon used by Harvey himself in the way of illustration (p. 129 of the Epistola Secunda ad Riolanum), I may adduce in the way of illustration also, being, as it is, dependent upon a similar nervous mechanism; and being shown so unmistakeably, in cases where it follows lesions in the nuchal region, to result from paralysis of nerve-centres situated there or thereabouts.

vation for the entire sympathetic system occupy but a small space at the base of the brain, two strips to wit, one on each side of the median fissure in the floor of the fourth ventricle; of, in the rabbit, a length of about four millimeters, beginning about four or five millimeters anteriorly to the calamus scriptorius, and ending about one or two millimeters behind the level of the corpora quadrigemina. The title of such a book as Eulenburg and Guttmann's Die Pathologie des Sympathicus auf Physiologische Grundlage (Berlin, 1873,) is an encouragement to those who hope to see fruit arise from such researches as these in the way of additions to our means for meeting, or at least understanding, human disease and suffering.

It has long been known (Budge, 1855) that the sympathetic nerves which supply the vessels of the head and iris do not pass directly or by the shortest possible route to this their distribution, but pass down the spinal cord for a greater or lesser distance, and then turn outwards, and pass from the anterior nerve-branches to bend upwards,

much as the recurrent laryngeal nerve does. That other vascular regions receive their vaso-motor supply by this apparently circuitous and, till the history of development is taken into consideration, paradoxical route, is from time to time being demonstrated. Dr. Pavy, to whom I have already referred, many years ago identified and mapped out one segment of the road along which nerve-force passes to the liver and prevents or allows the occurrence of diabetes. Further exploration of this route we owe to Cyon and Aladoff (Bulletin de l'Académie Impériale des Sciences de St. Petersburg, tom. xvi, p. 307; British Medical Journal, December 23, 1871); and this same investigator, working still in the same line of investigation, as it is in these days usually necessary for an investigator to work if he will make himself a name as a discoverer, has also shown us (Ludwig's Arbeiten, 3rd year, 1868) the track along which the vaso-motor nerves of the anterior limbs pass, proving that these nerves pass down in the spinal cord as low as the mid-dorsal region before leaving it to turn upwards in

the sympathetic chain to join the brachial plexus.

Of all the results, however, which have been attained to in the line of experimentation now under consideration, those come to by Brown-Séquard and demonstrated by him at the meeting of the British Association held at Liverpool in 1870, and subsequently published in the Lancet of January 7, 1871, seem to me to be certainly the most striking and possibly the most important. Could anything have been more surprising to him whose memory we here this day commemorate, than to have been told that an injury to a particular part of the brain, the pons, called after the excellent anatomist whose life ended in the very year in which his had begun, would produce haemorrhage in certain parts of the lungs, and anaemia, oedema, and emphysema in others? This is an easy experiment to repeat; it is one which might have been done in the days of Harvey as easily as in those of Bernard, of Budge, of Ludwig, and of Brown-Séquard. But easy though it would have been to perform, I am bold to say it was well for

Harvey that he never happened to perform it. For considering that, like Haller, he knew nothing of the contractility of arteries; considering that Hunter had not performed his now well-known experiments with the umbilical arteries; considering, Sir, that in that excellent work on Physiology by Johannes Müller, the translation of which in 1838, by our late and never sufficiently to be lamented friend Dr. Baly, we owe to your suggestion, I find several pages (vol. i, pp. 202–206, 214–219, ed. 1840) devoted to *disproving* the muscular contractility of arteries; considering, that it was not till three years later, in 1841, that Henle's work, already referred to, appeared with its still unsuperseded figures, Plate III, figures 8, 9, and 10 of the arteries with their circular muscular coat, and with its excellent summary in letterpress of the whole subject, pp. 518–526, and especially pp. 524, 525; when I consider that nothing of all this had been done, to leave unmentioned other advances connected with names of men yet living to speak for themselves and for us—I say it may have been well

that Harvey never came upon the facts relating to the alterations of lung-substance being entailed by destruction of brain-substance, not difficult to be observed and reproduced, which we owe to Brown-Séquard. For if he had come upon them, how could he have explained them in the absence of the entire chain of connecting facts, in the forging of which chain so many successive workers—Purkinje, Valentin, Weber, Burdach, Stilling, and others—have all contributed links? Might not even Harvey, often as he withstood such temptations, have, nevertheless, in default of power to assign the real causes of such a phenomenon, been driven back upon some of those explanations which he himself so forcibly denounces in the words (Epistola Secunda ad Riolanum, p. 116), 'Vulgo scioli cum causas assignare haud norunt dicunt statim a spiritibus hoc fieri et omnium opifices spiritus introducunt, et ut mali poetae ad fabulae explicationem et catastrophen θεὸν ἀπὸ μηχανῆς advocant in scenam.' It is a hard thing for any man to abstain from speculating as to the cause of any well-established phenomenon, especially

if it be of striking interest and importance; it is a hard thing for any man to do more than keep pace with his own generation; and those who have spent any time in reading the works of Harvey's contemporaries, will best appreciate the difficulty he must have had in setting himself free from the influence of the *idola theatri* referred to.

I pass from this reflection to an exposition of the claims which have been put forward on behalf of Walter Warner, the editor in 1631 of Harriott's Algebra, to the discovery of the circulation of the blood; and I do this by a natural transition, Walter Warner having been a man in whose mind, all his mathematics notwithstanding, the *idola* in question greatly abounded. Warner's claims are alluded to by Dr. Willis in a note to his excellent Life of Harvey (see p. lxiv). They are put forward by Anthony Wood, upon the authority of Dr. Pell, a man distinguished as one of Oliver Cromwell's diplomatists, and afterwards as an assiduous supporter of the then young Royal Society; and upon that of Dr. Morley, some time Dean of Christ Church, and afterwards Bishop of

Winchester (see Wood, Athenae Oxonienses, vol. i. p. 461, 2nd ed. 1721 ; vol. i. p. 302, ed. Bliss). Aubrey, a contemporary of Wood's, appears, from a note at p. 417 of the second volume of his Lives of Eminent Persons, to have had the same story from Izaak Walton, who gave Dr. Morley again as his authority ; and Aubrey repeats the tale with certain additions, and notably with that of Dr. Pell's authority, at p. 577 of the same volume. The same story was pointed out to me by one of the officials in the Bodleian Library as being given in an anonymous biographical Miscellany to be found in the Rawlinsonian Collection, B 158, pp. 152–153. This MS. appears to be of the latter half of the seventeenth century, and its legend runs to the following effect. A certain Henry, Earl of Northumberland, being imprisoned in the Tower, did, for the better passing of his time, get several learned persons to live and converse with him; one of these men (whom, Aubrey tells us, *l. c.* p. 368, the world called the Earl of Northumberland's magi) was 'Mr. Warrener.' And the MS. proceeds, 'He was the inventor, probably, of the cir-

culation of the blood, of which subject he made a treatise, consisting of two books, which he sent to Dr. Harvey, who epitomised and printed them in his own name; he usually said that Dr. Harvey did not understand the motion of the heart, which was a perfect hydraulik. . . . . Dr. Pain, that very ingenious and learned canon of Christ Church, told me that he had seen and perused this book of Warrener's.' Finally, the excellent Biographia Britannica has embalmed Wood's and Aubrey's story, in the articles 'Harriott' and 'Harvey,' pp. 2542 and 2550, ed. 1757. Many *à priori* improbabilities will at once be seen to attach to this story, and it is easy enough to discredit more than one of the witnesses. But I have better than indirect evidence to bring forward, and I will have the agreeable mental exercise of excogitating it to the ingenuity of my hearers, which ingenuity will be sharpened, no doubt, by their regard for their own Harvey, and strengthened by the belief that

> 'Whatever records spring to light,
> He never shall be shamed.'

I may be asked, after this quotation, why

I should have thought it worth while to investigate Walter Warner's claims at all. I will shelter myself, in the first instance, behind the example of Sir George Ent, who, feeling and acting by Harvey as Launcelot in his better days felt and acted by Arthur, took similar pains to set aside the similar fable as to Harvey's indebtedness to Father Sarpi. And, in the second place, I will remind my hearers that it was but as recently as 1838 that an article appeared in the London and Westminster Review, in which the claims of the Italian monk just mentioned were once again brought forward with surprising confidence, plausibility, and ignorance.

It was possible, I thought, that the same paltry but evil spirit which animated Dutens in writing his Inquiry into the Origin of the Discoveries attributed to the Moderns (1767[k]), and in coming to the conclusion

[k] Dutens was as well acquainted with the excellent work of William Wotton, Reflections upon Ancient and Modern Learning, published in 1694, on the other side of the question, as a little bitter mind can ever be with a work or the working of a noble and generous one. His repeated references to it show this, as also the unimprovable character of his shallow poverty-stricken spirit.

that every great man in modern times had been anticipated by somebody or other in ancient ones, might still be going about in dry places, and might wholly enter into and entirely fill up the soul of some small antiquary, who, coming under such inspiration and guidance upon the passages which I have collected, might proceed to instruct the literary public as to Warner's claims. Whilst considering what indirect evidence might be brought together to rebut this possible attempt at detraction, I came upon what led me to the discovery of the direct evidence I have promised to lay before you, in the shape of a clue which brought me, after a somewhat tortuous course, upon Walter Warner's actual autograph MS. I found, whilst following up Dr. Pell's history, scattered through Dr. Birch's unindexed History of the Royal Society, that Dr. Birch had procured a number of MSS. of Mr. Walter Warner's for that Society mixed up with Dr. Pell's (see vol. ii. p. 342 ; vol. iv. p. 447). Coupling this statement with the voucher for Warner's claims, ascribed by Wood and Aubrey to Dr. Pell (who, however, is never

reported in Dr. Birch's History, so far as I found, to have given currency to this statement), I thought that by these MSS. I should be able to test the truth of these statements. But the librarian of the Royal Society knew nothing of any MSS., either of Pell's or of Warner's; and, as the result will show, it would have been odd if he had—at least, in his official capacity. I then made inquiry of the Duke of Northumberland, in whose library the MS. of Warner, once a pensioner of his house, might possibly be preserved; but Mr. J. E. Martin informed me that this hope was a vain one. I found that Sion College had once possessed one MS. of Warner's; but I learnt from the Rev. W. W. Milman that they had lost it, and much besides, in the great fire of London in 1666. Finally, when taking the register of Merton College up to the British Museum for the purpose of comparing the entries made in that volume during Harvey's wardenship with his one authentic autograph MS. now in the national collection, I bethought me of making, at the same time, some inquiries as to Warner and Pell; and at last, when I least expected it,

and had nearly ceased to hope for it, I came upon Walter Warner's MS., contained in Dr. Birch's collection (which, according to him, had been made over to the Royal Society), under the title, '4394, Birch Collection,' numbered on in continuation of the Sloane Collection.

Mr. E. Maude Thompson, by the employment of various scientific methods, the observation of which went some way to compensate me for the tedious labour entailed upon me by the result to which they brought him, identified the MS. as being really Warner's, and even in bringing its date down to a year close upon 1610, half-a-dozen years or so, therefore, before Harvey first lectured at the College of Physicians. The MS. being thus identified I set myself down to look through its 416 folio pages, the average number of lines in a page being thirty-three or thirty-four; the average of words, many of them idle ones, being eight or nine in a line. I do not think it is very likely that I have missed any clearer exposition of Warner's views than the one which I am about to read from page 138; nor do I think that, by

choosing it, I can in any way misrepresent them, for they are stated elsewhere in the treatise in very much the same words, e. g. page 137. These, then, were his views:—'By this spontaneall pulsatory motion the bloud is continually extracted from the vaines (propter fugam vacui) as well originally exsuctory as secondarily circulatory and propelled into the arteries (propter fugam penetrationis), but with some diversity in the distribution, some part thereof being propelled up into the head by the internal jugular arteries, ad plexum choroideum for spirito-faction, the rest into all the rest of the arteries in universum corpus for organo-faction. Out of that part of the blood that is propelled by the jugular arteries up to the head, the spiritus confusus or immersus thereof being expressed and segregated in plexu choroidi, either by excussion or exhalation, and animal spirits, thereof made by the self-operation of the prae-existent in somno, it is again distributed as before, one portion thereof being still derived and transmitted to the heart, ad motum spontaneum pulsationis ciendum, and so about again, perpetua circu-

latione durante fabricâ corporeâ, and all violent destructions or impediments abstracted.'

It is, perhaps, needless to dwell further upon Warner's claims—certainly I do not propose to trouble you with reading to you any more of his speculations and conclusions. I have, however, had a copy made of folio pages 140, 141, 142, 194, and 195, and, though the gift may not seem a very valuable one, it will enable any fellow of Harvey's College to satisfy himself abundantly, and within our own walls, as to the real merits of the claimant before us, if the College will allow it to find a place in their library. In the words of Harvey's favourite poet,

> 'His saltem accumulem donis et fungar inani
> Munere.'

In all seriousness it is something to know what a contemporary of Harvey, and he a mathematician of some eminence, could write only some ten years before the actual demonstration of the circulation of the blood was given to the world.

Let me say, however, that I do not think it by any means impossible that Harvey may have read this treatise of Warner's, hard

though the labour of gathering hints, or rather warnings, from its many guesses must have been to him. For in many parts of Harvey's treatise, De Motu Cordis, we meet with phrases which seem as if they had been used with a special reference to Warner's views; and his dissertation has at least this claim upon my gratitude, that it has made me think that I understand Harvey's meaning the better for having read it. I fancy, in fact, that I recognise such phrases in Harvey's words (De Motu, pp. 58, 61, ed. 1766; p. 56, ed. Willis), 'Absque dolore vel calore *vel fugâ vacui*,' and in such words as 'longè plus est quàm partium nutritioni congruens est,' p. 64; 'αὐτοψία, non mentis agitatio,' p. 133. He might have been alluding to almost any page of Warner's MS. in his repudiation (p. 116, see Epistola Secunda ad Riolanum) of the hypothesis of various sorts of spirits. But there is one of Harvey's many noble and candid, whilst measured and well-balanced utterances, which seems to me to be admirably suited to serve as a text for an exposition which perhaps some future Harveian orator may undertake, of the exact relation which his

discoveries held to the knowledge and the ignorance, not only of Walter Warner, but of all others of his contemporaries or predecessors. These words run thus (De Motu, p. 34, ed. 1766 ; p. 33, ed. Willis) :—'Sed et hoc' (viz. the transmission of the blood by the action of the heart, from the veins into the arteries, through the ventricles of the heart into the whole body), 'omnes aliquo modo concedunt et ex cordis fabricâ et valvularum artificio positione et usu colligunt. Verum tanquam in loco obscuro titubantes caecutire videntur et varia subcontraria et non cohaerentia componunt et ex conjecturâ plurima pronunciant ut ante demonstratum est.' This may be translated thus :—'But it may be said, that all competent persons accept these views in a more or less modified form, and have been convinced of the truth of them from the structure of the heart, and the contrivance, position, and use of the valves. But they seem to me to make as little use of their eyes as men do who are stumbling about in a dark place, and their account of the matter is made up of heterogeneous, contradictory, and incompatible

statements, and very much of it is pure guesswork, as I have already shown.' These words, the Latin ones, not my translation of them, were published, if not written, nine years (see p. 5, Harveii Opera, ed. 1766, Dedicatio) and more after Harvey had first proved the facts of the circulation, and from them we gather that his discovery had, even so early as that date, got out of the stage in which a discovery is considered to be untrue, and got into that in which it is said that everybody knew it before. In no subject could it have been easier to make out a plausible case than in this of the circulation of the blood. Piccolhomini (an acquaintance with whom I owe to Mr. Walter Warner, see his treatise, pp. 194, 200, 201) had given a diagram, it is there before you[1], copied from the copy of his work in our library, of the junction of the portal and hepatic twigs, incorrect enough, no doubt, and obtained by a false method (see Harvey, Epistola Prima ad Riolanum, p. 105, ed. 1766), but still something in the way of a

[1] I have not thought it necessary to reproduce it in a woodcut.

working hypothesis (see Piccolhomini, Anat. Praelect.; Romae, 1586, p. 117, and Warner, MS. p. 194). Servetus had speculated, but rightly, as to the lesser circulation; so had Caesalpinus; and on Harvey's own showing (p. 15, and ed. Willis, ed. 1766), Realdus Columbus; and Walter Warner, p. 132 (4394 Birch Coll. MS.), had spoken of the heart, in 1610, as being 'a mere muscle, very strongly and artificially woven, and contrived with omnimodal nerveous fibres, direct, transverse, and oblike, as it were of purpose, for dilatation and contraction, according to the fashion of other muscles.' And of the action of the auriculo-ventricular and arterial valves, Harvey himself, *niveâ animâ*, with untarnished sincerity, repeatedly (see De Motu, pp. 14, 51, 53, 67, 81), speaks as of something known to all men, 'id omnes norunt' (p. 44). What then, it might have been triumphantly asked, was there left for Harvey to discover, when the action of the valves of the heart, its muscular character, and so much else, was already to be found in the writings or teachings of his predecessors? To all this we can answer, as

indeed, it seems probable, was practically answered even within Harvey's lifetime, what was left for Harvey to discover was nothing less than *the circulation itself*. His predecessors had but impinged, and that by guesswork, upon different segments of the circle, and then gone off at a tangent into outer darkness, whilst he worked and proved and demonstrated round its entire periphery. His demonstrations and direct proofs were all new, and his indirect arguments nearly all new. Whenever he made use of anything already known, he most punctually acknowledged it. Of his demonstration in the way of injection I have already spoken; of his demonstration of the use of the valves in the veins, and his proof that they are similar in function to the arterial, a fact previously unsuspected (see p. 65 *l. c.*), the thirteenth chapter of the treatise De Motu speaks with figures; of his indirect, but irrefragable argument, in the eighth chapter, from the quantity of blood thrown out by the heart at each pulsation, an argument which a mathematician such as Harriott, or Warner, might have hit upon, but, so far as I have

found, did not, he speaks himself as being 'adeo novum et inauditum ut verear ne habeam inimicos omnes homines;' and finally, the argument, which though it be indirect, every morphologist will allow to be not only most exquisite, but also most convincing, for the circulation in the adult warm-blooded animal, drawn from the relations held by the venae cavae to the efferent arteries in the embryo, and in all animals with but a single or an imperfectly divided ventricle, 'unus duntaxat ventriculus vel quasi unus,' and of which I would recommend every one who is not already acquainted with it, to gain a knowledge from the seventh chapter of the same book, was his, and his alone. With regard to all these points, with regard, that is, to the circulation as a whole; with regard to the actual demonstration and exhibition of it as opposed to mere guessing about it; with regard to all, or nearly all, sound reasoning as to any large portion of it, Harvey might have said with Lucretius,

'Avia Pieridum peragro loca nullius ante
Trita solo.'

or in the words of a poet of another country, and a later age,

> 'We were the first that ever burst
> Into that silent sea.'

I do not wish to assert that Harvey was wholly independent of the works of his predecessors; he himself would, as his repeated references to them show, have been the very last man to make any such claim for himself; nor would I say that he owed nothing to the times—

> 'The spacious times of great Elizabeth'—

in which he lived. It is true, I think, in science, as it is also true in morals and politics, that the times make great men as much as great men make the times. Many metaphors have been used to express this latter half-truth. Such is the metaphor, an acquaintance with which I owe to Mr. Picton's new and striking work The Mystery of Matter, p. 265, used by St. Augustine, in which great men are compared to great mountains, dwelling apart in loneliness, and sending floods of blessings down upon the little hills and plains at their feet. Such,

again, is the metaphor used by Wordsworth in apostrophising Milton:

'Thy soul was like a star, and dwelt apart.'

Such is the metaphor used by Sir Coutts Lindsay, in his poem on the Black Prince, where a hero

'Stands like a beacon, throwing light far out
Over the rippling tides of centuries.'

Now all these metaphors strive, and profess, to express but half a truth, and they are imperfect even for this imperfect purpose, as they are borrowed from inorganic nature and the arts, and are unfit to be used as illustrations of the complexities of life and thought. I would venture to suggest a metaphor which has struck me, during this investigation, as being more appropriate and close-fitting, even if less beautiful, than those which I have quoted. A group of horsemen are attempting to cross an arm of the sea, up which the tide has been running, and obscuring the ridge, or spit of sand, by which it is fordable. They form themselves into a line, and advance slowly: rider after rider flounders off into deep water, and, if wise, retires towards the rear of the cavalcade of

his companions, who still feel and advance upon the bottom beneath them. The line by degrees narrows into a column, and the column, after a longer interval, narrows into a single file. To the foremost horseman courage is necessary, as imagination is to the discoverer, and, impelled by this feeling, he may put a wide interval between himself and his companions, and reaching the opposite bank long before them, may have leisure to look down upon them, may be looked up to by them and by the rest of the world, whilst for some time in solitary occupation of that vantage ground. Such I conceive to be a fair representation, in the way of metaphor, the best and shortest way, perhaps, of representing such complex relations, of the relations held by Harvey, and indeed by most or all discoverers, to their contemporaries, to their compeers, and to the conditions whereby they are surrounded.

It may be expected, perhaps, that, coming from Oxford, and having been recently elected a Fellow of the College—the Wardenship of which Harvey held for something more than a year (April 1645 to midsummer

1646) — I should have made search for whatever records there may be left of him unpublished in Oxford, and especially in Merton College. After diligent search, I have to report that there is but little to be learned of Harvey's history from any unpublished document which I have been able to find in Oxford. The Merton College Register gives the following account of his election to the Wardenship. In 1645 King Charles I, after the execution of Archbishop Laud, took upon himself the functions of Visitor, and, having removed Sir Nathaniel Brent from the office of Warden, for having joined 'the Rebells now in armes against' him, he directed the Fellows to take the customary steps for the election of a successor. This course consisted in giving in, after due inquiry, three names to the Visitor, in order that one of the three, the one we may suppose it would be understood who was named first, should be appointed by the Visitor. Harvey was so named by five out of the seven Fellows voting; and, after a dispute of which it is unnecessary to give an account, he was duly elected on receipt

of a second letter from the King. A couple of days after his admission to the office, on April 11, 1645, Harvey summoned the Fellows into the hall and made a speech to them, to the effect that it was likely enough that some of his predecessors had sought the office of Warden to enrich themselves therefrom, but that his intentions were quite of another kind, wishing as he did to increase the wealth and prosperity of the College [m]. He finished his address to the assembled Fellows with an earnest appeal to them to cherish that mutual concord and amity amongst themselves, which recent occurrences, we may suppose, had tended to weaken. In the other pages of the Register for the period between April 1645 and the midsummer of 1646, I find the name of Charles Scarborough, the *protégé* of Harvey, and afterwards frequently an office-

[m] I would here remark that it was well perhaps for the College of Physicians that Harvey was, by the success of the Parliament, forced to vacate the office of Warden, otherwise he would, no doubt, have kept his word, and Merton College would have gained what the College of Physicians, or some others of his legatees, would have lost.

bearer in this College; but there is little or nothing of special interest to us in the rest of the record, beyond the fact that Harvey appears to have attended the College meetings and so to have discharged his duties, amongst which the providing for the contingency of a siege and famine was one. Mr. Pettigrew (Medical Portrait Gallery, 1840) has put on record the fact that Harvey's signature is to be found in the Liber Computorum of Merton. The College Register, however, is not so enriched, as I can state upon the authority of Mr. E. Maude Thompson, who compared the pages relating to Harvey's wardenship with the autograph MS. in the British Museum, when I took the Register up to London for that purpose [n].

[n] Mr. E. M. Thompson has made another search for Harvey's missing MS. De Anatomiâ Universâ, which Dr. Lawrence mentions at p. xxxi. of his Harveii Vita (ed. 1766), and which Dr. Willis tells us at p. vii. of his Preface (ed. Willis) had then (1847) been twice looked for in vain. Mr. Thompson's search has also been equally fruitless; he writes to me thus, under date June 3, 1873:— 'Harvey's Anatomy *was* once upon a time in the British Museum. In the first volume of the old MS. Catalogue of

Of Harvey's, as of Berkeley's sojourn in Oxford, we know little; little, indeed, has

the Sloane MSS. (now marked Sloane MS. 3972 A), there is this entry on p. 57 :—

"C. $\frac{450}{230}$.—Praelectiones anatomicae universales per me, Gulielmum Harveum medicum Londinensem Anatom. et Chirurg. Professor. Anno Dom. 1616, aetatis 37, praelect. April 1, 1617."

To which is added, "This is the author's foundation and first Lecture of the circulation in his own handwriting," and opposite to it is this note by Sir F. Madden, "In the place of 230 (which seems missing) Ayscough substituted the bracketed no. (6)." So you will see from this that the MS. was missing in Ayscough's time. I have ransacked our MSS. without finding any clue; so I think you may make up your mind that it was borrowed, and has gone the way of borrowed books in general.—E. Maude Thompson.' Wood says (Fasti Oxonienses, ii. 6) of Harvey, 'But more in MSS. hath he left behind him, the titles of which you may see in the Epist. Dedicat. before a Historical Account of the Colleges Proceedings against Empyricks' (1684, London, Ch. Goodall). Moved by this authority, though Goodall only says that Harvey *designed* these treatises, I looked over a large number of medical MSS., assisted herein by Mr. Walter de Gray Birch, in the Sloane Collection of the British Museum, without the desired result. Subsequently I found that Harvey himself in 1650 (p. 163 and p. 502, ed. 1766; p. 148 and pp. 481, 482, ed. Willis) had recorded the loss of his 'adversaria multorum annorum laboribus parta,' and especially of his work De Generatione Insectorum, when his house was plundered in the Civil War. Later

been recorded, with the exception of the somewhat uncertain gossip of the gossiping Aubrey. But what we do know of the place during those years which elapsed between the battle of Edgehill and 1646, makes us certain that scientific, and indeed any other work, must have been carried on in it under great disadvantages. We read of the plague, and of the 'morbus campestris,' described by a former Harveian orator and Linacre lecturer as desolating the town and driving people out of residence. It was, besides, a centre for military operations; and military life has been shown, by the experience of all ages (though this experience appears to have been lost upon the heedlessness and ignorance of this), to be out of harmony with the habits of men, old as was Harvey then (aet. 64–68), young as our undergraduates are now, who are, or who ought

again I came upon the following passage in Lower's work, Tractatus de Corde, ed. 1669: 'Quid quod et Harveius si per aetatem et otium licuisset plura polliceri videtur ipse, Lib. de Circulat. Sanguinis, cap. 9. . . . . *Sed quod maxime dolendum est et ille voto suo et nos spe nostra excidimus.*' Hence I fear there is now little hope either of recovering or of discovering the lost MS.

to be devoted to study. Whatever else of Aubrey's tales of Harvey I may disbelieve, I can believe that the words addressed to Charles Scarborough, 'Prithee leave off thy gunning and stay here,' are his.

If, however, we wish to have a real and truthful picture and image of Harvey before us, we must do by him as we have to do by Shakespeare, by Aristotle, by Butler, and several other great writers: we must lay our minds alongside of his, as it is revealed to us in his works. It is only the writings of great men which will bear or repay such treatment: no commentary nor any biography can give us the real and vivid sensation of having the men before us which we get from a perusal and reperusal of their books. Having used for this purpose what Mr. Tom Taylor has recently spoken of[o] as 'the invaluable three hours before breakfast,' I have come to persuade myself that I have obtained something like a trustworthy idea of what Harvey really was. Previously, however, to doing this, I gave Christian burial to

[o] See speech at Eighty-fourth Anniversary Dinner of the Royal Literary Fund, 'Times,' Thursday, May 29, p. 12.

much of what Aubrey has left on record about him, feeling more and more strongly as I grew better acquainted with Harvey that—

> 'These were slanders: never yet
> Was noble man but made ignoble talk.'

I will speak first of his scientific *character*, though it may seem strange to speak of scientific *character*, as *character* implies, perhaps, a moral element; and science, so far as it is really science, and based exclusively upon sound reasoning, has no moral element in it; reasoning, so long as it is sound, being of one kind always, and devoid therefore of all distinctive or personal factors. It is necessary for me to say that I do not forget that Harvey was but eighteen years junior to Bacon,

> 'Whom a wise king and Nature chose
> Lord Chancellor of both their laws.'

But neither do I forget that the Novum Organon was published in 1620, subsequently to the discovery and actual demonstration of the circulation (see Dedicatio to the treatise De Motu Cordis), if not to the publication of the treatise on the Motion of

the Heart; and that the Royal Society, with its motto, 'Nullius addictus jurare in verba magistri,' was a foundation of a much later date. And consequently, I think, we may feel justified in saying that, so far as the purely scientific factor of a man's nature can be said to have any distinctive or personal character at all, independence, or robustness, or manliness, whichever word we may like to choose, as shown in superiority to mere authority and the weight of great names, was a distinctive character of Harvey as a man of science. With Riolanus in full vigour, and Van der Linden growing towards maturity, as champions of antiquity, it required not a little manliness to assert, 'contra receptas vias per tot saecula annorum ab innumeris iisque clarissimis doctissimisque viris' (Riolanus was often thus spoken of), 'tritam atque illustratam' (Dedicatio, p. 5), the claims of simple Nature 'quâ nihil antiquius majorisve auctoritatis' (Epistola Secunda ad Riolanum, p. 123). This element of real manliness shows itself again, I think, in Harvey's power of abstaining from suggesting a rationale of what

he felt he did not understand; as, for example, in what is known (out of England, at least) as the 'Problem of Harvey' (see De Partu, pp. 132, 549, ed. 1766; p. 530, ed. Willis)—a problem which, I think, could not have been answered till the 'works and days' of Bernard[p]; and in the cases of

[p] I refer to Claude Bernard's experiments on the influence of vitiated air (Des Effets des Substances Toxiques et medicamenteuses, 1857, p. 125), which show so plainly that organisms can attain a power of tolerance as against morbific agencies if time is allowed them to become gradually adjusted to such environment. The principle demonstrated in these experiments has been brought into greater prominence by Sir James Paget in his striking account (Lancet, June 3, 1871; p. 734), so interesting to all of us for other than purely scientific reasons, of his serious illness in 1871. As regards the 'Problem of Harvey,' the *foetus in utero* has been habituated to lowly arterialised blood; the blood of the umbilical vein is not scarlet in colour, and hence, I submit, may be explained the tolerance by a child which has come into the world but has not yet breathed in it, of conditions which entail death by suffocation in a child which, *having breathed air*, is exposed to them. This physiological principle has, among many other practical bearings, the practical value of furnishing an answer to the Philistine argument so often brought forward by Antisanitarians in favour of the retention of abuses, in the words 'see to what a good old age people live in the middle of it all!' The answer is,

several other problems instanced by himself (p. 132, Epistola Secunda ad Riolanum), and hidden then, to use his own metaphor (p. 630, ed. 1766; p. 613, ed. Willis, Epistola Prima ad Horstium), in the well of Democritus.

For the culture which Harvey had bestowed upon his literary faculties, we have better evidence than Aubrey's, better even than that of two more trustworthy witnesses than Aubrey—Bishop Pearson, to wit, and Sir William Temple: we have the evidence of his own writings as to his familiarity with one of the greatest writers of antiquity. Bishop Pearson, as Dr. George Paget has reminded us (see p. 15 of his Notice of an Unpublished Manuscript of Harvey, 1850), writing in 1664, but seven years after Harvey's death, and Aubrey (see p. lxxxii. of Life by Dr. Willis, prefixed to the Sydenham Society's edition of his works, 1847), have

'They have become habituated, and are living *in spite of*, not *because of* these surroundings: immigrants die in the process of acclimatisation.' Such persons, and indeed all persons, may read with profit Mr. G. H. Lewes' Physiology of Common Life, vol. i. pp. 372–377, upon this subject.

told us of Harvey's high appreciation of Aristotle's writings; but in his own writings he refers to the Stagirite more frequently, I think, than to any other individual. And, as regards Vergil (the Latin author whom probably, if but one Latin classical writer could be saved from destruction, most men would choose to be that one, as Aristotle probably would be the similarly to be chosen Greek), Sir William Temple (Miscellanies, Part ii, On Poetry, p. 314) has told us that 'the famous Dr. Harvey, when he was reading Virgil, would sometimes throw him down upon the table and say he had a devil.' It was a similar spirit which dwelt in Sir Philip Sidney, who never heard the famous ballad of Percy and Douglas without feeling his 'heart moved more than with a trumpet.'

It may seem to some but a small matter to vindicate for our great discoverer claims to a familiarity with Greek; still, any one who will look at such passages as the one in the Exercitatio de Partu, p. 553, where he speaks of the mischief done by meddlesome midwives, or other passages (pp. 116, 129, and 133, Epi-

stola Secunda ad Riolanum; p. 613, Ep. ad Slegelium), will see, I think, that he had Greek in abundance at his command, and used it just when it helped him to express his thoughts more clearly and concisely than any other words at hand at the moment. He used it, in fact, like a man of sense and real learning, when the use of it would save him time or trouble—two things, of one of which he had all too little, whilst of the other he had all too much for his and our good. Let me add that, in the one authentic MS. which we now possess of Harvey's (No. 486, Sloane Coll., British Museum), a MS. never intended for publication, and consisting but of rough notes for lectures to be delivered, I find that he employs Greek words in several places (e. g. pp. 65, 66 and 87) [q].

[q] I have no sympathy with the eagerness which scientific men sometimes (see Fritz Müller, Für Darwin, p. 28; Dallas' Engl. Trans. p. 42) show in repudiating a knowledge of Greek, but on the other hand I should be sorry to be thought to overrate its value. I am so far from doing this that I incline to thinking that, when through want of leisure or of means, or through some other deficiency, a young man cannot add on more than a second

His style has been spoken of as being more or less inelegant and unadorned; and the Latin tongue which he used lends itself but grudgingly and awkwardly to the purposes of science, being strictly a political language, habituated and framed to describe the march of the legions, the disputes of the forum, or the denunciations of the moralist. Still, Harvey's style has always an impressiveness and solidity of its own; and sometimes, as for example in the glorious eighth chapter, De Motu Cordis, it rises into real eloquence where a great occasion justifies the use of repetitions, of antitheses, and abundance of metaphors. But, though the use of stilted phraseology was common enough among Harvey's contemporaries, and though his imagination was vivid and active enough, his study (for to this perhaps we may ascribe it) of the excellent models mentioned saved him, as such a study can

foreign language to his acquirement of Latin (which I presuppose), that second foreign language should, in the case of Englishmen, be, for linguistic and educational, as well as for more lowly practical reasons, not Greek but German.

save a man, from falling into the use of false or extravagant imagery.

Harvey, besides the advantages accruing from acquaintance with the great minds of the past, enjoyed also those which may be gotten from familiar intercourse with great contemporary minds. These advantages constitute in themselves a second education; and they were at Harvey's command for the period of more than forty years during which he was prominently before the public. It is recorded as one of the many distinctions of John Greaves (see Life, by T. Smith, 1699, p. 44), the once celebrated astronomer and antiquary, and a man whom we can well believe to have done more, as a Fellow of Merton, than give a silent vote for Harvey when he was chosen Warden, that he was one of the friends of Harvey as well as of Archbishops Laud and Usher. It is indeed in a letter to this latter dignitary, and in answer, we may suppose, to an appeal from him on behalf of Harvey, that we find John Greaves pledging himself in a postscript, under date Sept. 19, 1644, the year before Harvey's election as Warden of Merton, to

the following effect: 'If I may serve Dr. Harvy (*sic*) I shall be most ready either here or at Leyden to do it.' (See Life of James Usher by Richard Parr, D.D., 1686, p. 510[p]). His well-known connexion with

[p] I owe this last reference to the Biographia Britannica, *sub. voc.* Greaves. For a further account see Wood's Athenae Oxonienses, vol. iii. ed. Bliss, 1817. To the former of these sources I owe a second and more interesting reference, viz. to Birch's edition, 1737, of the Miscellaneous Works of John Greaves, where, at the end of Greaves' Treatise on the Pyramids (pp. 136, 137), we have given us an account of a conversation between him and Harvey. It runs thus: 'That I and my company should have continued so many hours in the Pyramid and live (whereas we found no inconvenience) was much wondered at by Dr. Harvey, his majesty's learned physician. For, said he, seeing we never breathe the same air twice, but still new air is required to a new respiration (the *succus olibilis* of it being spent in every expiration), it could not be but by long breathing we should have spent the aliment of that small stock of air within, and have been stifled; unless there were some secret tunnels conveying it to the top of the Pyramid whereby it might pass out and make way for fresh air to come in at the entrance below.' The Fellow of Merton was not wanting in an answer to the future Warden, assuring him, amongst much else not wholly correct, that 'as for any tubuli to let out the fuliginous air at the top of the Pyramid none could be discovered within or without.' Harvey replied, 'they might be so small as that they could not easily be

the court must have constantly brought him into relation with the statesmen of those stormy times. His legacy to his 'good friend Mr. Thomas Hobbs, to buy something to keepe in remembrance' of him, is touching, even if trifling, evidence in the same direction.

Travel, which even in our day confers a kind of culture peculiar to itself, must have

discerned, and yet might be sufficient to make way for the air, being a thin and subtil body.' It has, indeed, been left to our own times and to v. Pettenkofer to demonstrate and exhibit the action of the capillary pores in the constituents of a mass of 'solid' masonry (see his Beziehungen der Luft zu Kleidung, Wohnung und Boden, 1872, pp. 41–45, and especially the figures p. 42). What Leeuwenhoek and Malpighi did for the capillaries of the animal body in supplementation of Harvey's work, and in correction of one of his few errors, that v. Pettenkofer has done in supplementation of Harvey's suggestion as to 'tubuli so small as that they could not easily be discerned' in structures like the Pyramids. It is, perhaps, not more than curious to note that Harvey was equally right in suggesting the existence of larger 'secret tunnels': an account of the discovery and opening of them may be found in Colonel Howard Vyse's Operations carried on at the Pyramids of Gizeh, 1837, i. pp. 3, 263, 285–288; ii. 160, 161; and an amusing history of the inconveniences endured in the interior of the Pyramids previously to the discovery of these 'air-channels' is given by Colonel Coutelle in Description de l'Egypte, Antiquités, Mémoires, ii. 46, 1809.

been doubly necessary in days when, in the absence of the steamship and the railway, an insular position must have kept its inhabitants very nearly as inaccessible to 'the thoughts that move mankind,' as it had happily kept them to the Armada. Sir George Ent's interesting and entirely trustworthy account of the interview with Harvey which resulted in the publication of the treatise De Generatione, will show any one who will consult it that Harvey had drawn from his opportunities an insight into what might be expected, and what since his time to some extent has been realised, from enlarged opportunities of observing not only 'men, manners, cities, climates, governments,' but also the wonderful facts of the unequal allotment, in the various parts of the earth, of useful inorganic products, and of that mystery of mysteries, the distribution of organic life. (See Works, ed. 1766, p. 162; ed. Dr. Willis, p. 146.)

Having been thus fortunate in securing for himself all the advantages which the various educational agencies of his age would furnish, he added on to all that they

had effected, or could effect, the yet more elevating and glorious discipline of long sustained and finally successful labour. He attained a position of mental dignity in which he could feel neither unduly anxious for the applause of his compeers, nor unduly moved by the reproaches and misrepresentations of his enemies (see Dedicatio, p. 164; Epistola Secunda ad Riolanum, p. 109); the impact of these opposite forces resulting, however, in much benefit for mankind, as without them Harvey might, it is likely enough, have delayed the publication of his works indefinitely. Being self-contained without being self-conscious, he was yet, like all men of real genius, large-hearted and sympathetic. Whilst he could, in a spirit of perhaps a little overstrained charity, make excuses (see p. 614, Epistola ad Slegelium) for the pestilent and irrepressible Riolanus, he would, we may be also sure, have felt an emotion of gratitude upon each of the many instances in which his own true-hearted adherents, Sir George Ent and other Fellows of this College, fought his battles for him, and vindicated for him suc-

cessfully and during his own lifetime his own irrefragable claims. And I can believe that, answering to the character of the dignified, stately, and high-minded man so well drawn by the author whom he often quotes (Aristotle, Eth. Nic. iv. 3 (7)), and considering himself worthy of great respect, being worthy of it, he would not have looked disapprovingly upon our attempt to show him respect by the Tercentenary Memorial to which you, Sir, have lent the sanction of your name. I can further conceive of Harvey as entirely sympathising with the men who have now in their hands the torch of knowledge which once passed through his, of applauding without any shadow of jealousy the work of the many workers who in these days are going over the ground trodden by him under far less favourable circumstances and with far less assistance from ancillary [q]

[q] Such an experiment, for example, as that put on record by Professor Haughton (Principles of Animal Mechanics, 1873, p. 151), as performed by Professor Macnamara with his assistance, and as showing that the time occupied by absorption, circulation, and secretion occupies less than four minutes, requires the employment of iodine; and iodine has been discovered and isolated but some sixty-two years.

sciences and their various and still novel instruments and methods. The same spirit which caused him repeatedly to say (as, for example, to Sir George Ent, p. 163; to Horstius, p. 630), 'haec cum mirâ, ut solet, promptitudine effundens,' that he doubted not that much now hidden in darkness would be brought to light by the indefatigable industry of the coming age; the same spirit which dictated the provision in his will bidding 'his lo. friend Mr. Doctor Ent' sell certain of his 'books, papers, or rare collections,' and, 'with the money buy better,' would have caused him, could he have been amongst us, to point out, as a matter for congratulation, in how many directions his discoveries had been extended and added to, and how well replaced had been the many works the loss of which had been so 'crucifying' to him.

There was not in Harvey's mind that defect in the way of a deficiency of interest in theological questions which constitutes in the minds of some eminent scientific, and some eminent literary, men such a lamentable void. He has, on the contrary,

in several places taken pains to state his views upon this highest of subjects. To one of these passages (from the work De Generatione Exercit. Quinquagesima, p. 385, ed. 1766; p. 370, ed. Dr. Willis), as Mr. E. B. Tylor has pointed out to me, Professor His of Leipzig, a worker whom Harvey would have hailed as a colleague, has referred in one of his always excellent papers, published in the Archiv für Anthropologie, Bd. iv. 1870, p. 220, on 'Die Theorien der geschlechtlichen Zeugung.' It is just in the investigation of the problems indicated in these last words that, as has often been remarked, the question of the existence of other than purely material forces presses itself most closely upon the mind; and hence, perhaps, the repetition by Harvey of his views regarding it, more than once or even twice, in his treatise just referred to (see Exercit. 49, p. 730; Ex. 50, p. 385; Ex. 54, pp. 419, 420). These statements are all to the same purpose. I have chosen one of them—the last one of the three just cited (not the one quoted by Professor His)—to repeat here, because, besides its philoso-

phical and other interest, it has some literary claims upon our attention, it being not quite impossible, considering its line of thought and arrangement of words, that Pope, who borrowed on all sides, and made acknowledgments on none, may have had it before him when he composed his Universal Prayer. It runs thus :—

'Nempe agnoscimus Deum, Creatorem summum atque omnipotentem, in cunctorum animalium fabricâ ubique praesentem esse, et in operibus suis quasi digito monstrari; Cujus in procreatione pulli instrumenta sint gallus et gallina. Constat quippe in generatione pulli ex ovo omnia singulari providentiâ, sapientiâ divinâ, artificioque admirabili et incomprehensibili exstructa ac efformata esse. Nec cuiquam sane haec attributa conveniunt, nisi omnipotenti rerum principio; quocunque demum nomine id ipsum appellare libuerit: sive mentem divinam cum Aristotele; sive cum Platone Animam mundi; aut cum aliis Naturam naturantem; vel cum Ethnicis Saturnum aut Jovem; vel potius, ut nos decet, Creatorem ac patrem omnium quae in coelis et

terris; a quo animalia eorumque origines dependent, cujusque nutu sive effato fiunt et generantur omnia.' (De Generatione Animalium, Ex. 54, pp. 419, 420, ed. 1766; p. 402, ed. Willis.)

I have detained you far too long; but, feeling that my praise of Harvey has been all too feeble, I am anxious, in ending, to employ in honour of Harvey certain lines of singular beauty and force which, though composed in commemoration not of him, but of another famous Englishman, may nevertheless be applied to him with a singular appropriateness.

'Remember all
He spoke among you, and the man who spoke;
Who never sold the truth to serve the hour,
Nor paltered with Eternal God for power;
Who let the turbid streams of rumour flow
Thro' either babbling world of high and low;
Whose life was work, whose language rife
With rugged maxims hewn from life;
Who never spoke against a foe.

. . . . . .

Whatever record leap to light,
He never shall be shamed.'